Response to Reviewer #2 (Remi Tailleaux)

I thank the reviewer for the constructive and helpful review. In this response, I respond to all points raised and describe how I will accommodate these in the revision of the manuscript.

In the following point-by-point response, I will state the reviewer's comment in *italics*, followed by my response and description of the action that I will take in the revision.

Major comment 1: Free energy versus exergy versus APE. *I find the discussion of the concepts of free energy versus exergy versus APE somewhat confusing and unclear. In Tailleux (2013) cited by the author, I interpreted exergy as the available energy defined relative to a state of thermodynamic equilibrium with uniform temperature, and APE as available energy defined relative to a state of minimum potential energy obtained from the actual state by means of an adiabatic re-arrangement of mass as per Lorenz (1955) theory. In my mind, there is a significant difference between the two, as transforming exergy into useful work cannot be done without simultaneously destroying exergy and creating entropy irreversibly. In contrast, APE can be converted into useful work (kinetic energy) without the need to destroy any of it nor creating entropy irreversibly. In this regard, APE appears to be a `freer' form of free energy than exergy. Can the author try to highlight the differences and interrelations between the different concepts? Does the author define free energy relative to thermodynamic equilibrium, or does he also consider that APE is a form of free energy? An advantage of the exergy concept is that it provides a clear explanation of where the free energy comes from, i.e., from the convexity of the internal energy. If internal energy was not a convex function of entropy, it would not be possible to transform `heat' into `work'.*

Response:
Thank you for this elaboration. What I describe in the manuscript is more or less consistent with what the reviewer writes, so his comments can be addressed by a revision. In principle, free energy (or exergy) can be described as the fraction of energy within a system that, once dissipated, produces the entropy to bring the system from disequilibrium into a state of thermodynamic equilibrium and maximum entropy. This is equivalent to what the reviewer writes, and I will elaborate on this in section 2.2 (see also response to Reviewer 1).

For the Earth system, this reference state of thermodynamic equilibrium is, however, not so meaningful. This is because the planetary forcing in form of differential, radiative heating and cooling always generates temperature differences, and a state of thermodynamic equilibrium at the planetary scale is never achieved. Hence, it seems to be more meaningful to think of free energy budgets in their respective steady states, and these are represented by generation and dissipation terms - that is, fluxes of free energy, rather than stocks of free energy. When it comes to the generation of free energy, it can be seen from the derivation of the Carnot limit in section 2.3 that this generation is associated with an energy flux with no entropy as it does not enter the entropy budget.

Regarding the comment on APE, I think this can easily be rectified by noting that, essentially, the sum of available potential energy and kinetic energy represent both free energy, and the relative proportion of each of the components can freely be converted - just as it is the case in the dynamics of non-dissipative waves.

In terms of the "advantage of the exergy" concept, I would like to comment that the notion that free energy comes "from the convexity of internal energy" is quite a mathematical way to put it. In this manuscript, I prefer to focus on the physics - and here, free energy comes from the disequilibrium associated with the frequency distribution of photons across wavelengths associated with solar radiation at the Earth's orbit.

Action:
I will clarify these points by extending description in Sections 2.2 and 3.1. I will clarify the definition of free energy and the specific context of the Earth system as being a flux-driven system that includes a budget equation for free energy. The point about APE is clarified in section

3.1. The point of solar radiation being at disequilibrium at the Earth's orbit will be clarified in section 2.5.

Major comment 2: Dissipation/generation*. It is not clear to me what the author means by 'dissipation' or `generation'. As regards dissipation, does the author mean 'viscous dissipation' or does his definition also include APE dissipation by diffusive processes, which Tailleux (2009) argued should be regarded as an additional form of Joule heating similar to viscous dissipation? The concept of APE dissipation seems important, because for simple fluids, it is proportional to the rate of irreversible entropy production by diffusive fluxes of heat, which means that there is a link between the production of entropy production due to the destruction of APE (viewed as a form of free energy) and the entropy production due to passive or dynamically inert heat diffusion through the background reference stratification. As regards 'generation', it seems to me that it is an ambiguous term. Indeed, I'd like to point out that in the oceans, for instance, oceanographers still do not agree on how to define the power input due to surface buoyancy fluxes as discussed in Tailleux (2010).*

Response:
These are good points! Yes, dissipation of kinetic energy (i.e., friction) represents viscous dissipation in the context of the paper, that is, the conversion of kinetic energy back into heat (or, microscopically, less random motion of molecules becomes random). The thermal dissipation part, e.g, by heat diffusion, would already be captured by the entropy production term in the entropy budget that was used to derive the Carnot limit. After all, thermal dissipation is not associated with the generation and dissipation of free energy.

In terms of APE dissipation, I think this can be seen to apply to the atmosphere as well, although this dissipation is done by the mass flow associated with the heat flux that drives the atmospheric heat engine. At present, I do not see how it is, however, relevant to the atmosphere. I therefore suggest that I mention this concept and the difficulty to clarify power inputs for the ocean briefly at the places in the manuscript where these things are being discussed.

Action:
I will describe dissipation in Section 2.2 more clearly and mention that such free energy budgets are not as simple as they may sound, as, e.g., the definition of power input for oceans is still being debated. The concept of APE diffusion and its role for oceans will be mentioned at the end of section 3.1.

Major comment 3: *On the role of moisture. I believe that the author makes an important point in pointing out that the climate system differs from the kind of heat engines considered in textbooks in that the net heating and cooling are not fixed but to be obtained as part of the solution. Regarding the role of moisture, the author may be aware that Laliberte et al. (2015) have argued that moisture reduces the efficiency of the atmospheric heat engine relative to a dry one. It would be of interest if the author could comment on this and whether he agrees or disagrees from his perspective.*

Response:
Thank you for raising this question! The main difference of how atmospheric heat engines and the hydrologic cycle is being described here is that these processes are described including the energy balances of the system, and, as the reviewer rightly describes, that the heat fluxes are not fixed, but constrained. This picture results in a view that hydrologic cycling is a part of the turbulent fluxes that are derived from the maximum power limit (as in Section 2.4), and that the intensity of cycling is set by the thermodynamic equilibrium conditions (Section 3.2). The power output from dry convection is thus certainly reduced, which is consistent with Laliberte et al. (2015), because a part of the turbulent fluxes is represented by evaporation. Yet, as evaporation rates can be predicted very well by this approach, the magnitude of hydrologic cycling also in the atmosphere is strongly constrained by the surface input, and by the thermodynamic constraints described here. It would thus actually seem that - similar to section 3.1 - the surface inputs

already contain the strong constraint on hydrologic cycling within the atmosphere that makes it predictable from thermodynamics, as well as the response to global climate change.

Action:
I will add a paragraph of discussion to section 3.2 to discuss the relationship of the Laliberte et al. (2015) paper and related papers on thermodynamics and hydrologic cycling.

Major comment 4: *Carnot efficiency versus maximum power efficiency. The author cites a number of studies related to maximum power. Wouldn't it be relevant to cite endoreversible heat engines and the ideas of Curzon-Ahlborn here?*

Response:
That's a good point! The Curzon-Ahlborn efficiency assumes that there is a dissipative loss term in a heat engine due to diffusive heat exchange at the system's boundaries. These loss terms are not considered here because atmospheric heat engines operate mostly with direct radiative heating and cooling, in which heat does not need to diffuse into the engine from a hotter source or out to a colder sink. The diffusive laminar layer right at the interface between the surface and the air is rather small and also not a fixed property of the engine, while the radiative cooling of the atmosphere has no diffusive layer across which the cooling takes place.

Action:
I will add this clarification to the discussion in the manuscript in section 2.3, which derives the Carnot limit.

Minor comments:

Abstract. I find the first sentence to be particularly obscure. Could the author be somewhat more specific? Second line: what does 'it plays' refer to? If this refers to 'thermodynamics and optimality', plural should be used.

I will reformulate the first two sentences of the abstract to clarify.

Lines 23-26. Could the author be more specific about the `simplicity' of the examples discussed? This seems to be a bit subjective and left to the appreciation of readers.

I will modify the second paragraph and be more specific.

Line 87. Might be important that entropy increases irreversibly only in closed systems.

Understood - this will be reformulated, although it is important to note that even for open systems, entropy can only increase, but this takes place outside the system.

Lines 180. Free energy budgets are important. It seems to me that these are widely used but not called that way. What about APE budgets or the kind of available energy budgets considered by Bannon and co-authors for instance? Could the author be more specific about what he has in mind exactly? What is his definition of free energy?

Agreed. I will expand the description, as outlined in the response to Major Comment 2.

Line 184: `These all can be formulated in terms of free energy budgets, a concept that is rarely used in Earth system sciences' I am not sure that this is true, as for me, APE budgets or budgets of available energy represent 'free energy budgets' so the author needs to explain what a 'free energy budget' would look like and provide examples.

Well, this sentence does not state that free energy budgets are not used at all, just that they are rarely used. I'll revise this paragraph and make the description more explicit. See also response to Major Comment 2.

Line 233 – This derivation of the Carnot limit is general and quite different to common textbook derivations. I am a bit puzzled by this statement, as this is the derivation of the Carnot efficiency I am used to, and the one I was taught as an undergrad.

I'll rephrase this paragraph.

Line 240 – is given by G = D. This may be the case but because I think that there is no consensus about how to define G and D unambiguously in all cases (see my remarks about APE dissipation above), as is the case in the oceans for instance, it is unclear how to link MEP and ideas of maximum power or dissipation in the most general case.

The goal of this manuscript is not to provide a general recipe on how G and D are being described - after all, this likely contains process-specific information. The examples in Section 3 actually show that heat engines primarily apply to the atmosphere, but other processes may nevertheless quite closely related to these heat engines (like evaporation and carbon uptake, examples described in Sections 3.2 and 3.3). I will include this clarification when introducing free energy budgets in Section 2.2 (see also related responses above).

Lines 275-277. What about endoreversible engines and the Curzon-Ahlborn efficiency?

As described in response to Major Comment 4, I will add a comment on the Curzon-Ahlborn efficiency already a bit earlier when the Carnot limit is derived in Section 2.3.

Lines 376 – Against friction. As well as against APE dissipation may be.

In the atmosphere, as far as I know, it is essentially the friction in the boundary layer and at the surface that dissipates the kinetic energy of the large-scale atmospheric circulation. I'll make this more specific in the revision.

Lines 385-390. This assumes that we understand how to define and quantify both power and dissipation in all possible cases, but I don't think this is true, e.g., discussion in Tailleux (2010) for the oceanic case.

This description does not claim to apply in all possible cases but deals specifically with the large-scale atmospheric circulation in this section, with no reference being made to the ocean. Also, the MEP applications of Paltridge dealt with the atmosphere. I do not think that it needs further clarification, so I suggest no alteration of this section.

Line 443 – Here the author uses the term 'frictional dissipation' where he only used 'dissipation' before. See the need for clarifying the term 'dissipation' in major comments above.

This term will be more clearly explained in the revision. See response to Major Comment 2.

Line 450 – I agree that this is a particularly important point that I think will need to be more fully recognised and expanded upon in the future.

Thank you! I guess this does not need modification, so I will leave it as it is.

Line 463 – 'so that these conversions neither generate nor dissipate free energy' This is true only for APE but would not apply to exergy for instance, whose transformation into useful work requires destruction.

As I am not referring to exergy (or free energy) here, but to descriptions of the Lorenz Energy Cycle, I think this sentence does not need adjustments.

Line 527 – GPCP – missing reference

Will be fixed.